

**CIE Review of the Biological Opinion on the Effects of
Ocean Salmon Harvest on Sacramento Winter-Run
Chinook Salmon**

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Executive Summary

The Biological Opinion for ocean harvest on Sacramento River winter-run Chinook salmon is largely based on the reconstruction of harvest rates of hatchery salmon, and the estimation of the loss of spawners those fisheries have caused. The technical analysis of harvests is based on a CWT cohort analysis that is fairly typical for Chinook salmon. It appears the cohort analysis may underestimate the impacts of the fishery on sub-legal sized fish. Further documentation of the assumptions of model and a sensitivity analysis to evaluate uncertainty in the results is recommended. The data used as inputs into the model and some of the outputs should be provided in tabular form. Further analysis of effort and catch/effort data would be informative for evaluating fishery regulations. In my opinion, the inclusion of these revisions should result in a robust document.

The second component is the Biological Opinion where the estimated fishery impacts are evaluated in relation to ESA criteria for viability of the population. The BO is a less-well developed document that is in need of significant revision. The major shortcoming of the BO is a lack of detailed analysis of the impacts of the current fisheries management regime on the population, and in particular how it would be evaluated relative to the Viable Salmon Population criteria. The BO makes reference to an abundance-based management regime for California salmon fisheries and the potential for this regime to enhance the viability of the winter-run population. An analysis of the covariation in abundance and survival of the various California populations is required, as is an analysis of the effects of fishery regulation on SRWC impacts. Finally, further analysis of effort and catch/effort data would be useful for supporting inferences drawn about management regimes and impacts on SRWC abundances.

Background

Sacramento winter-run Chinook salmon are a unique population of salmon that historically spawned in the headwaters of the Sacramento River basin. Construction of various dams and water projects has resulted in the collapse of sub-populations into one mainstem spawning aggregation below the Shasta Dam. The abundance of the stock has declined, and in recent years spawner counts have cycled, presumably due to changes in ocean conditions. Declines in the 1990s led to the population being listed as endangered under the Endangered Species Act (ESA).

Sacramento winter-run Chinook salmon are caught incidentally in ocean fisheries off of the California coast that are directed at more abundant populations. The impacts of the fishery are estimated using a cohort reconstruction based on CWT recoveries of hatchery fish from the fishery and spawning areas. A Biological Opinion for the winter run has been prepared to evaluate the effects of past and proposed salmon fisheries on the survival and recovery of the population.

The Review

The CIE review of the impacts of the ocean fishery on winter-run Chinook salmon focused on O'Farrell et al. (2010), a draft document that describes the cohort reconstructions and impact evaluations, and the Biological Opinion (BO). The BO contains a large body of background and supporting information that was not reviewed as it was not central to the issue of the ocean fishery impacts. A third document, called the Consultation Package was briefly reviewed and used for supporting information.

As the 2 main documents are in draft form, detailed comments related to the scientific content, analysis and presentation were prepared. From these comments summary responses to the 5 questions provided in the TOR were developed. The reader should note that much more detail on responses to the 5 questions are found in the detailed document reviews. The analyses and models of Chinook fisheries in the Pacific Northwest were used for comparison, and references for the documents that were used are provided. This review was office-based and was conducted in Vancouver BC.

Responses to the Statement of Work Questions

- 1. Evaluate the strengths and weakness of the cohort reconstruction analysis used to estimate the impact of fishing harvest on the ESUs considered in this Opinion.*

The strengths of the SRWC analysis come from the relatively simple context that the run is situated in. With only one major spawning aggregation, two fisheries with simple regulations, and a reasonably high tag and sampling rate the structure and

quality of information likely exceeds many other situations. The methods used for the reconstruction are fairly standard for Chinook salmon and could be considered equivalent to the best available methodology and practice.

The main weaknesses of the reconstruction analysis are:

- A lack of detail of some of the data, input parameters, analysis and assumptions, as well as detail in some of the outputs in O'Farrell et al. that weakens the presentation from a scientific perspective.
 - An inability of the approach to estimate impacts due to contacts with age-2 fish when none of these fish appear in the catch. Non-retention impacts should be presented in a manner similar to catch so that effects of past and proposed regulations can be evaluated.
 - Lack of explicit analysis of effort and catch/effort data.
 - Failure to consider the loss of spawners after river entry.
 - The lack of consideration of uncertainty in the approach, this could be minimally be dealt with using a sensitivity analysis.
2. *Evaluate the interpretation of the coded wire tag recoveries and cohort reconstruction analysis, and any conclusions drawn about how these results are produced in light of how the fishery is managed.*

The intent of this question is somewhat unclear. The interpretation of the cohort analysis "in light" of how the fishery is managed is difficult because the documentation does not provide a clear listing of the various restrictions put on the commercial and recreational fleets over time, nor are the changes in effort and technology well documented (Appendix C of the consultation package does provide some insight into the changes that have occurred). The results of the cohort reconstruction are effective for estimating population level impacts, but as noted below, I think that a more detailed analysis of catch and effort by region and month (and year, given regulatory changes) can be used to make more specific recommendations the management of the fishery.

3. *How could the cohort reconstruction data or analysis be improved?*

See my comments from (1) above. The main way the analysis can be improved is though better support for the some of the assumptions of the model. This includes more analysis of the sub-legal catch, sensitivity analysis to evaluate uncertainty, and further work on effort dynamics.

4. *Are there additional quantitative or qualitative ways other than coded wire tag based methods to assess harvest impacts not considered in this Opinion?*

CWT methods are generally used along the Pacific Coast for estimating fishery impacts, survival rate and other parameters, but factors such as cost and mark-selective fisheries are leading to agencies reconsidering this approach (see

http://www.psc.org/info_codedwiretagreview.htm). Genetic stock identification (GSI) may provide some additional information on the composition of the catches, especially if there is concern about using the hatchery stock as a surrogate for wild production. It may also provide better estimates of impacts in strata where only a few CWTs are being captured as untagged population is much larger than CWT group. GSI analysis also uses only a small piece of tissue, so concerns about mutilation of landed fish are not an issue. GSI cannot easily be used to estimate survival rates, however, as the size of the outmigrating population is unknown. Modern GSI labs can provide turnaround in only a few days, so real-time management is quite reasonable.

A more detailed analysis of the dynamics of the fishery may also be useful for evaluating regulatory measures. In the reports provided for this analysis it is apparent there exist detailed effort and catch data, and the analysis of this information could help both regulatory efforts, as well as providing some information about the distribution of the stock in coastal waters (by evaluating CPUE).

Sub-legal catches and impacts caused by releasing them does not seem to be handled well in the analysis, and it would seem that some ancillary information should be brought to bear on the issue. I did not have time to dig up literature from the California fishery, but am aware of an older study of the Chinook fishery in the Georgia Strait BC region that is informative (Argue et al. 1983). At the time of the data analysis for that project (the late 1970s) there was a 12 inch limit for sport and 18 inch for troll. The size at ages for Chinook salmon are similar between the BC and SRWC populations. For the BC fishery the summer troll catch was 10-40% age-2 fish, and the sport fishery was 50-65% age 2s during the summer months. I cannot comment on the differences in gear and technology between the 2 datasets, but information presented in Argue et al. (1983) suggest that the catch of sub-legal fish (i.e., age-2s) in the California fisheries should be quantified as the release mortality could represent a significant impact. Observer data, or perhaps information from time periods when the size limits were lower may be needed to estimate the vulnerability of age-2 salmon to the gear. This suggestion assumes that age-2 are exposed to the fishery, of course.

Overall, does the biological opinion represent the best scientific information available?

I have provided a number of technical comments on the cohort analysis that might make only made minor differences to the final outputs. Consideration of all the reviewers' comments will likely improve the "scientific information". However, the cohort analysis is retrospective in nature, and describes impacts of the fishery as the result of the management regime that occurred over the past decade. It is does not seem possible to predict the impact of future regulatory changes (other than the status quo or full closures) from the analysis as no attempt was made to compare impacts to the dynamics of the fishery or the changes in regulation. This may be difficult to do if the fishery were more or less constant over the years as is implied

by the relatively constant impact rates. However, the comments at the end of the BO suggest that a more active management regime is in use. A more detailed analysis would be required to evaluate the impacts of an abundance or forecast-based fishery management regime.

Thus, in light of the lack of analysis of the fishing regime that is currently in place, I cannot support the statement that the BO represents the “best scientific information available”. Rather it seems that the major conclusion of the BO, that under an abundance-based harvest regime impacts to the SRWC are minimized (or is considered acceptable), is rather anecdotal and unsupported. There is a relative imbalance in the level of analysis that was provided in support of the BO compared to that used for the cohort analysis.

A key element of the fishery regulations is the early year closure of ocean fisheries. On page 41 of the Consultation Package it notes “closure...during February through March... is expected to reduce the overall harvest...by 28%”. This statement does not acknowledge the fluid nature of fishing effort and the lack of analysis of effort patterns. The data presented show that in the early 1970s 28% of the harvest took place in February and March, but it is not clear that these closures would necessarily result in a 28% reduction in effort or harvest if those regulations are enacted some 40 years later after these data were collected. More rigorous thinking and analysis is needed to predict the relation between regulation, effort and impacts in the BO.

Comments on O'Farrell et al., cohort reconstruction.

This document provides the technical analysis of the magnitude of “impacts” to the SRWC hatchery component that is used in formulation of the biological opinion. The report is well written and the analysis appears to be fundamentally correct, although there are a few outstanding issues that need to be clarified.

My main comment on this report is that it lacks enough detail and raw data in a number of places that prevents the reader from clearly understanding the analysis and the assumptions that underlie it. See CTC Chinook Technical Report TCChinook01-2 (in particular see Page 3) for a list of assumptions for a similar model; many of which these also applicable to the case.

Secondly, the paper does not consider uncertainty in any of its components or the final outputs. Many modern stock assessments now use methods that explicitly include uncertainty in assumed input parameters and the data sampling process. However, these can be quite challenging to implement in a time-limited environment. Rather, I would suggest that the authors conduct a sensitivity analysis of the key parameters of the model and tabularize them so that the reader can be assured that the main findings of the model are not hinging on one or 2 key assumptions. For that analysis it may be sufficient to consider the sensitivity of the main output, the mean SRR or impact (rate) to changes in some of the input parameters or assumptions. For example, vary M from 0.1 to 0.3 (compared to the baseline of 0.2). If the likely range of an input values has only has a small effect on the SRR, then the conclusions of the BO can be considered robust to some of the uncertainties in the cohort analysis.

There can be a significant number of encounters with age 2-fish in some Chinook fisheries that are all going to be sub-legal in the current management regime. The cohort reconstruction cannot account for these contacts unless there is some landed harvest (and CWTs) from that age class (because equation 3 goes to zero when harvest is zero). The model needs to account for this, unless it has been empirically demonstrated not to be an issue because of gear types or fishing practices, or because the age-2 year class is not exposed to the fishery. Some more discussion of impacts on small, young salmon is needed.

Following from this point, a description of the ocean distribution of the stock would be very helpful for considering the fishery impacts. Based on juvenile surveys do the age-0 and age-1 fish have a wider and more offshore distribution than age-2 and age-3 fish? Are age-2 fish distributed differently than age-3/4s? How far offshore does fishing occur by the troll and sport fleets?

Some detailed comments:

Page 3, last paragraph: Clarify that the analysis is for the impact of ocean salmon fisheries on the hatchery component of the SRWC. No analysis is conducted on incidental catches in other fisheries.

Page 4: Some CWT tag loss can be expected, a recent estimate averaged 6.7% for adult Chinook salmon (Knudsen et al. 2009). It is assumed here that the tag retention after release is 100%. It's likely that the tag loss occurs early in life, so tag loss could be incorporated into the $1/\phi$ correction. I think this will only affect the release to age-2 survival rate calculations.

Page 7: The explanation of the expansion for the Keswick trap is not that obvious. I assume that all fish collected at the trap are marked and the CWTs are all decoded? Do the Jolly-Seber estimates and the proportion of tags in the carcass recovery allow a second estimate of the hatchery component that is somewhat independent of the methods reported in Appendix C?

Data—general: It would be useful for the reader to present some tables of the key data that are not included in the Appendices. This could include annual estimates of hatchery and wild fish in the escapement (and trap), as well as the actual CWT numbers by age/gear/location (see next comment). Tables of fishery effort would also be useful.

Page 8: Specify this is ocean **salmon** harvest. If possible, provide some support for the assumption that the incidental harvest in other non-salmon fisheries can be ignored (or has to be ignored due to a lack of information).

Pages 8-9: Some summary statistics about the proportion of the catch or landings that are sampled are useful to assure the reader that there's satisfactory coverage.

Page 10: Later in this report there is mention of the small numbers of CWTs in some strata, and a scan of the Appendix E tables reveals that this is not an insignificant issue. It would be useful to create a summary table of counts of CWTs from Table E-1, stratified by year, age etc. as estimates based on 1-2 tags recovered are going to be very uncertain.

Page 10:

2.2.1 Cohort reconstruction: This is the heart of the analysis, and a little more explanatory text is needed to explain the approach and what each equation is for. My comments on the equations (by equation number) are:

(2): Here it is assumed that there is no mortality of adult spawners after river entry other than fisheries retention. This assumption needs to be made explicitly and justified, as there are now many examples of in-river mortality of adult salmon that are very significant (see Cooke et al. 2004, for example). Increasing river temperatures seem to be a significant issue in some rivers and have led to direct physiological impacts or have resulted in disease outbreaks. I don't know if this is an issue in the Sacramento, but some comment is required. Similarly, drop-off and sublegal retention and release mortality are not incorporated in the recreational river fishery. If these are minor such that they can be ignored, then that should be stated.

(3): This equation does not seem correct. I would have thought that the contacts would be the Harvest + Drop offs + Sub-legals(released)+Legals not retained (I assume the latter is zero). Equation 3 does not appear to account for the fish hooked but not landed. Assuming the drop-off rate is the same for all size fish, I think equation 3 should be $C = (\text{Harvest} + \text{Drop-offs})/p$ by age. I assume the number of dropoffs can be approximated as $\text{Harvest} \times \text{dropoff rate}$. Incidentally, the CTC (1997) has used values of 2.5% for barbless hooks in commercial troll fisheries, and 6.7% for recreational fishing. No values are provided in O'Farrell et al. although reference is made to values in the other documents.

The assumption that is being made in the current model is that vulnerability is constant within an age-class regardless of size. Based on the analysis of Argue et al. (1983) the assumption may be justified although local information would be useful. Also note that the definition of p in Table 1 is incorrect, p is the proportion of fish in the population larger than the legal limit (based on the Appendix). The ocean harvest will have a similar distribution if there is no size bias in their vulnerability to the gear.

Equation (3) won't be useful in cases where there are contacts but no harvest of an age class. Thus, in the absence of an age-2 catch, there will not be any estimated contacts (in fact, the equation is undefined when $p = 0$ (i.e. all fish of an age class are below the size limit)). It seems that some ancillary information on size-based vulnerability is needed to estimate the contact rate on age-2 sublegal fish. These fish are relatively abundant (20-40% more than age 3s), and some surely will be hooked during fishing by either fleet unless they are unavailable to the fishery or invulnerable to the gear. Perhaps there is something about the capture of age-2 fish that is well known by the salmon community in California that I am missing.

(4): This equation appears to account for the mortality of sub-legal fish once contacted (landed?) and released. This needs to be spelled out on page 10.

(5): This is the drop-off mortality in numbers of fish. The problem with this equation is that C , the contacts, does not include drop-off fish because it derived from only the landed catch (H) in equation 3.

In addition to the suggestions above, it might be worth adding some more text to explain the need for the various equations in this section by explaining Harvest, Release Mortality, Drop Offs etc. There is also the assumption that 100% of the legal fish caught in the recreational fishery are landed- is this actually the case? Are there bag-limits, or self-imposed catch and release efforts that would increase the contacts relative to the harvest?

(8): You might note the assumptions made about the occurrence of both natural and fishing mortality. When the time intervals are relatively short and the rates of loss relatively low, then the approximation used in equation 8 is reasonable. However, when natural and fishing mortality occur simultaneously over a longer period, the exact formulation or Pope's approximation is required. The model also assumes that annual mortality is evenly distributed throughout the year.

(9): The correction ($v_a/(1-v_a)$) is unusual, as the normal way to use M (or v_a) is in the form $\exp(-M)$. They do lead to slightly different outcomes, and the version in the paper is increasingly incorrect at higher values of M . Can the motivation for this formulation be included, relative to standard methods such as described in Hilborn and Walters (1992) or Ricker (1975, p11), for example?

Page 11: The parameter values used in the reconstructions can have a large effect on the size of the at-sea population and the impact of harvest on that population. The report should have a separate section on parameter values- including what values were chosen and why. I note the mortality schedule used by the Chinook Technical Committee (CTC) of the Pacific Salmon Treaty is somewhat different than the one proposed here although an age2+ mortality of 0.2 has become widely accepted since its early estimation by Ricker. The use of 0.5 for the age-2 mortality needs to be justified.

Inputs for the calculation of p values are also needed (fishery size limits) or, provide a table of p values by age, month, gear. There is not enough information here or in the Appendix to evaluate the parameter values used. Based on the data in Table A-1, it would seem likely that troll contacts with sub-legal sized fish could be significant given the size limit (26") is the mean size of an age-3 fish in summer (meaning half of the age-3s and all of the age 2s are less than 26"). However, from the Appendix in the Consultation Package the size limits have frequently been changed in the recent past—is this accounted for in the analysis?

Finally the values used for drop-off rates and release mortalities are not specified as far as I can tell, although a variety of rates are discussed in the BO and consultation package. This can be a contentious issue, so detail and justification is needed.

Page 12: Equations 13-15. It be noted that the area-based rates are the impact of the fishery in that area on the whole population, not the harvest (or impact rate) on the segment of the population in that area. Some fisheries may have a lot of effort in an area, but have low impact on this population if the fish are elsewhere.

Equation 16 is somewhat of an approximation since the bulk of the fishery occurs three to six months after the March 1 accounting date. To put the impacts at the same time frame the numerator should be adjusted by the expected survival so that it is in March 1 equivalents (i.e., expand the impacts as I (i.e., the sum of all impacts for month i) * $\exp(v_a * \text{the number of months from March 1 to month } i)$). This gives a more accurate perspective of what the impact has been on the March 1 population. The current formulation underestimates the impacts by about 10% because there would be some mortality on the fish that were caught (from March 1) if they had not been harvested.

Page 12, the SRR analysis: To be more explicit—was E^0 calculated by taking the age-2 abundances from the cohort analysis and calculating forward using only natural mortality and maturation rates? It might be worth stating this and providing some equations so the reader can better understand what was done.

Page 13, Results: Each region of the Pacific Coast has its traditions and corresponding vernacular, but I would prefer to use the term mortality, rather than impacts; to me, impact does not necessarily imply mortality (for example, injuries caused by hooking might be considered an impact that might not lead to mortality). For example, the y-axis labels of Figure 1 could be number of fish, and the caption would be the mortality of fish due to the direct and indirect effects of salmon fisheries.

Page 13: The harvest size limits for each fishery should be available in this report. It might also be worth putting the number of CWTs available for each bar in Fig.1. My quick scan of Table e-1 suggests that the annual age-4 estimates are based on 1-6 tags with the exception of 2005.

Figure 4: Some acknowledgement of the very small number of CWTs for most years and the uncertainty that creates is warranted in the text (the 2004 estimate is based on a single tag!). A little bootstrapping exercise or a more formal incorporation of binomial sampling distribution would assist in evaluating the uncertainty creating by having only a CWTs for these strata.

It would be nice to have a chart or table with escapement, river harvest and ocean impacts by year so the reader can place the ocean impacts in the context of the size of the whole population. This would be a useful place to also document the magnitude of estimated non-retention mortalities as well, as these could be quite significant and represent a loss to both the fishery and the stock.

Page 20: While the impact rates are useful for determining where and when SRWC are being removed from the stock, the data are in place for some further analyses of fishery impacts. In particular, the effort statistics could be represented by month and area, and then the average catch/effort could also be summarized. Because the “sampling” of the stock is decidedly not random or uniform, it is only CPUE that

provides an indication of where the stock is (at least within the coastal area that fishery operates within).

It is apparent from the observation that the impact rate has remained constant while the impacts (the number of fish killed) has fluctuated with stock size that effort has remained relatively constant, and the catchability of the stock is not affected by its abundance. This is not surprising given the regulatory regime and that the stock is a minor component of the overall fishery. However this is further support for a more explicit evaluation of catch/impact and effort as noted above. I note that an effort time series is presented in the BO, but there's room for more analysis here.

Page 20, age 2 reconstruction: The age-2 abundances also include variation and potentially biased caused by using a constant age-2 to age-3 survival rate (when the real survival rate is unknown and likely varies annually).

Page 24: First sentence- this assumes that no age 0 to age 2 salmon are caught in other fisheries in the region. Is this true? In some regions shrimp and groundfish trawls can be significant. Also, I would suggest the 2002 and 2003 hatchery releases were only "slightly larger" than average.

Figure 11: It is unclear why the Y-axis scale is so different among years. Are all data summed across years, and it's due to a different number of years in the 3 periods, or is stock size or effort different among periods? Wouldn't dividing by the number of years be a reasonable way to put the 3 panels on a similar basis?

Page 26: The comparison of harvest across the 3 stanzas (Fig 11) only allows a comparison of the distribution of harvest across months, not the "effect that ocean fishery regulations have had on the SWRC stock". For the latter, additional information on the distribution of effort and stock size are required (which also assumes some stability in technology/catchability to make comparisons across a 40 year period).

Page 26/27: As my point for page 20, it would be useful to report the "stability" of effort and regulation especially in the core areas. Has the recreational effort been relatively constant so that the "impact" is a result of variation in stock size, or is some of the variation in impact related to changing effort, fishing techniques, the weather (as it affects recreation fishing) etc.? This might also provide insight as to whether the distribution of the stock (or the fraction actually vulnerable to the fishery) varies from year to year.

Conclusions:

1. I am not sure that "stability" is meaningful. Why not "the average SRR over the study period was xx% with a CV of xx% and showed no trend" or something like that which is more specific?

2. Was an annual impact calculated for 1969-70 to support the conclusion that total impact has been reduced as a consequence of the closures? For example, effort may be relocated to the open period to maintain the overall impact. Perhaps all that can be said is that the closures have eliminated some of the seasonal patterns of exploitation.
3. This is a general statement that is usually true for salmon. It would be straightforward to present correlations (figures) of age-2 abundance, catch, impact and escapement to support this assertion. As it stands, it is more of an anecdotal observation.
4. This is more of a recommendation, but quite reasonable.

Comments on the “Biological Opinion on the Effects of Ocean Harvest...”

This document is an early draft and is in need of careful editing. The authors need to look carefully at each sentence and ask “what am I trying to say?” as there are a number of wordy convoluted sentences, as well as the liberal uses of slang and acronyms that are confusing to the non-initiated.

The document does present a lot of the background material for the population and its threats, but the actual “Biological Opinion” could certainly be enhanced with additional analysis beyond the cohort reconstruction. It also attempts to repeat many of the technical analysis of O’Farrell et al. by (re-)presenting some of the historical information (albeit some in a slightly different form). I would suggest that the some of these technical matters be reserved for O’Farrell et al., and only the key results be presented in the BO. For example, presenting raw CWT data in maps or figures is not useful here, and the discussion of parameter values for non-retention fish should be in O’Farrell et al. as these are inputs of the model.

Unfortunately this document does not have line or page numbers, so my comments are referenced by section and my count of the pages.

E. Sacramento River winter-run (Chinook salmon)

Table 3: This is a useful calculation to track changes in productivity. It would be reasonable to use an empirically derived age at maturity schedule (using averages for years where data is not available) to decompose the returns to brood years to create a more accurate estimate of productivity. From the text it appears that another source of error in the table is the lack of explicit consideration of hatchery production on the replacement rate.

E.2 Current viability: While the spreading of risk by the presence of multiple spawning populations is desirable, there are many examples among salmon populations of viable populations that spawn in a single location. These are commonly populations that are relatively large, and use stable, productive freshwater habitats. Many sockeye populations fit this description, as well as some fall-run Chinook salmon.

IV. Environmental Baseline, page 29: It is important to distinguish between the ocean distribution of the stock, and the ocean distribution of catch, which is a function of the distribution of both the stock and fishing effort. In the absence of the systematic survey of the ocean areas, it’s not clear what can be said about the ocean distribution of the stock. Both the BO and the cohort reconstruction document could use a little more information on ocean distribution or at least some detail about what is known and unknown. For example, is it not clear how far offshore each fishery extends, relative to the distribution of each age-class of the population?

V. Effects of the Action.

Page 37: The recovery of tags is contingent on at least 3 factors: The sampling rate, the number of fish in the region, and the fishing effort by area. Figure 7 is useful for visualizing the raw data, but inferences from this figure should be limited. The discussion of fishing effort on page 37 should be supported by a table of effort statistics by gear, month, and area. Citing a table in another report does not help the reader.

Page 37, paragraph 2: “It is interesting to note” – this is an example of where some rigorous editing of the document is needed. In the material in this paragraph, the point is made that catches that were made in the winter and spring have been eliminated by time-based closures. However from the information provided the statement “the success of management measures implemented to minimize winter run impacts... can be attributed to the elimination of fishing effort...” *cannot* be supported. First, impacts in these documents are defined as the number of fish killed by fishing, and the data of Figure 11 in O’Farrell et al. suggest that the impacts are higher in the recent period than in 1969-1970, probably because of the increase in stock size. The impact *rate* may have been affected, but that would depend on the dynamics of effort—has effort and catch shifted to the summer months (and has effort grown because of human population growth)? Has technology increased so that the catchability has increased? With the information provided it seems that all that can be stated is that fish are no longer caught in the winter months. Figures 11, 13 and 15 in the Consultation are also confusing because there are different numbers of fishing seasons involved in each figure, which affects the y-axis scale.

Figure 7 and 9: As noted above the recovery of CWT’s is dependent on a number of factors, and the discussion would be better supported by figures of impacts from O’Farrell et al. that take into account all the adjustments needed for sampling etc. To be complete “winter-run” should be “Sacramento River winter-run Chinook salmon” throughout the document.

Page 39: Why not plot escapements and impacts by year to support this paragraph instead of Figure 9? I cannot tell if the pattern in Figure 9 is a function of sampling rate, fishing effort or changes in abundance. As an editorial note, I found the 2-letter acronyms of the fishing areas to be quite distracting and would prefer to have these all spelled out. Locals may be more conversant in them, however. There are spelling and punctuation errors in this paragraph. Introductory phrases such as “for the most part” are unhelpful and should be removed.

2. Salmon ocean fishery

The start of this paragraph is an important point and needs to be supported with a figure that includes ‘impacts’ (number of fish) by gear and year, not CWT recoveries. The remainder of this paragraph is very confusing. It is argued in successive

sentences that the effort levels are not comparable but the differences in effort can explain differences in catch. Is figure 11 all stocks? There are going to be all sorts of differences between the 2 fleets that contribute to differences in catch and catch composition. Lastly, in the final sentence, what exactly is meant by the statement that the fishery been “depressed” (do you mean regulated?).

Page 40, bottom: Too many acronyms. For the outside reader, explain if there are other limitations to the fishery other than time closures (limited entry, gear restrictions, space closures). The remainder of this paragraph is hard to follow, and probably should be expanded to a more plain-English description. What does “quite a long while now” mean?

Figure 10: Caption—should be “Total effort in all areas south of Punta Arena, by year. Commercial fishing is effort by salmon trollers, recreational angler trips are.....”

Page 42: This first sentence needs to be rewritten and clarified. The end of page 42 and top of 43 is vague speculation and perhaps should be reconsidered. As noted above there are a legion of ways that fishing can differ between gear types and without more information and analysis, not much can be said. Using the results of O’Farrell et al. and some additional information on the vulnerability of sublegal fish one could evaluate the effects of the differential size limits on the catch by each fleet. If you want to do that, Table 5 should be removed and replaced with direct estimates of vulnerability, or at least use the values of p and abundance by age/month in O’Farrell et al. to discuss the effect of the size limits. Estimates of CPUE and effort can be used to evaluate the effectiveness of each fleet. Since O’Farrell et al. attempts to estimate the impacts of non-landed fish, I think this paper should focus on the estimated impacts instead of revisiting the assumptions of O’Farrell et al.’s analysis.

Page 43: The figure in Appendix B is the same as Fig. 8 of O’Farrell et al. The graphical presentation is useful but the discussion may be enhanced by a table of the Catch/Effort values (the slopes) that summarize the material in the table in terms of catch rates. Otherwise the reader is left with making a visual estimate of the slopes of the catch/effort regressions. This is not easy and probably not the best way to present information for a non-technical audience.

Page 43 very bottom: SFRC FMP, etc. acronyms. This paragraph could use a little more detail. Perhaps state that the SI is an index of abundance based on.... The SI harvest index appears to an approximation of the fishing mortality on the Sacramento Fall run (Sport+Troll?). It isn’t clear from the discussion what management options are used to affect the harvest index and how those measures relate to the variation in effort shown in Figure 10. Perhaps the material for Figures 10 and 12 can be consolidated to provide a more complete description of fishery management options and actions. The statement “likely proximately associated with” needs interpretation.

Page 45. Section 3. Non-retention

Why are Klamath fish mentioned here? Is it an assumption or observation that all fish that are landed are kept? For example, are there any catch and release sport fisheries? Has the 2 fish bag limit been in place throughout, and does this affect non-retention (via “high-grading”)? The presence of the bag limit is not mentioned in O’Farrell et al.

The fishery is well defined so that statements like “in general, the minimum size for the commercial fishery is larger than the recreational” can be replaced with the exact regulations.

It is interesting that a number of values for hooking and drop-off mortality rates are provided here but cannot be found in O’Farrell et al. Since O’Farrell et al. needs to be revised to include the parameter values used, this section should be able to provide the exact values used in the analysis. As noted in a number of places, an important issue that is not handled by the O’Farrell et al. report are the number of contacts of sub-legal age-2 fish. This needs to be resolved and the outcome of that analysis can be reported here.

Overall, this section can be made much more specific

Section 3 [sic] Cohort reconstructions

The term “winter-run” is an adjective; please add the appropriate noun.

Page 46, second paragraph: What is a “developing winter-run”?maturity rates ... *have...*

Please provide the evidence that age-3 fish are the most vulnerable to the fishery. This is an attribute of the distribution of the fish relative to the fishery, and the catchability of the size class. There are more age-2 fish than age-3 salmon in the water, and they would seem likely to be vulnerable to the gear, unless they are differentially distributed in the ocean relative to age-3s. Certainly age-3 fish are the most common in the catch but that’s not vulnerability.

It should also be noted that the age-4 impact rates are based on very few recoveries and are very uncertain.

B. Response

A lot more could be done with the SRR data. As noted in the Opinion, with a short life cycle (3yrs) the effects of additional spawners could accelerate population growth quite significantly. While it is true in theory that population growth rates may ultimately be limited by the amount of available and suitable habitat, no

evidence has been provided that this is the case. An escapement of 230,000 in 1969 (p. 19 of the Consultation) indicates that the capacity is probably much larger than recent values. In fact, for the analysis of many endangered species, habitat limitation is ignored because the population is far below the carrying capacity. As a base case, I would suggest incorporating the reproductive contribution of additional spawners under the assumption of no density dependence. Potentially, that analysis might show that the estimated impacts have reduced abundance by more than half of the unfished levels in the last 2-3 years. If there is evidence to support habitat limitation (likely in the spawning areas for ocean-type fish) that can be modeled as a constraint on large broods. Thus I disagree with the footnote (#22) on page 50.

Page 50: Please explain what a “typical and meaningful level” means.

VII Integration, Page 51:

This first paragraph is somewhat unclear, perhaps because of the jargon. There is a lot more than CWTs in the analysis of fishery impacts, so I would change reference from CWTs to the cohort analysis. Can you explain “seasonal restrictions of the proposed actions” (in the first paragraph). Then I would suggest “O’Farrell et al estimates that most of the impacts are due to the recreational fishery....”. What are the “anticipated level of efforts...”.

Figure 17 and 18—“returns” can mean many things. Is this estimated number of spawners in the river + broodstock, or something else? It’s unclear what from the caption or labels if Figure 18 is SRWC or a combination of populations.

Page 52: Evaluating risk of extinction when population dynamics are driven by a cyclical survival rate that takes the population above and below replacement levels is challenging, and I don’t think the calculation of the average lambda is terribly helpful. It is certainly true that populations can sustain themselves for longer periods of adverse ocean conditions if freshwater habitats are productive (maximizing smolts/spawner) and the initial population size is large.

What is the difference between a “systemic decline” and a “temporary adjustment”? Plain language is needed here.

It should also be possible to use the hatchery smolt-to-age-2 survival rates and the cohort replacement rates calculated from escapements to partition some of the variation in recent survival rates. If one assumes that the hatchery survival rates are correlated with wild rates, then the hatchery data could be used as an index of survival for the period of smolt migration and ocean residence, while the cohort replacement rates index both the spawning and incubation period plus the ocean stage. Similarities and differences in their trends might be informative. This information could further be improved if the returns could be corrected for harvest to yield a true recruits/spawner index (incorporating age structure as well).

The last part of the paragraph on page 54 is a little unclear. Were fisheries “depressed” or regulated?

The discussion on page 55 and 56 is extremely important to evaluating the future impacts on SWRC as implies that active yearly management of other, larger stocks would be beneficial for the winter run if the patterns in abundance and survival covary. It should not be difficult to determine the extent to which covariation does occur using assessment information for these stocks. The analysis could then form the basis for simulation studies to evaluate the effectiveness of harvest control algorithms for the aggregate. It would be especially useful if a leading indicator (such as age-3 returns to populations that have both age-3 and age 4 components) could be used as a pre-season planning tool, as is implemented for the Klamath population.

A management regime that tracks productivity would likely have much less impact on the population than one that invokes a constant 20% impact on the population regardless of changes in abundance or productivity (especially if the O’Farrell et al. analysis has underestimated impacts in the recent past). This is especially true if ocean conditions continue to vary in the highly autocorrelated manner that has been recently observed. Further analysis and simulation studies would be beneficial in building a stronger case that an adaptive harvest policy would could cause a significant reduction in risks to the winter-run Chinook population.

References:

Argue, A.W., Hilborn, R., Peterman, R.M., Staley, M.J. and Walters, C.J. 1983. Strait of Georgia Chinook and coho fishery. Can. Bull Fish. Aquat. Sci. 211.

Cooke, S.J. and many coauthors. 2004. Abnormal migration timing and high en route mortality of sockeye salmon in the Fraser River, British Columbia. Fisheries **29**(2): 22-33.

Hilborn, R. and Walters, C.J. 1992. Quantitative fisheries stock assessment. Chapman and Hall.

Knudsen, C.M., Johnston, M.V., Schroder, S.L., Bosch, W.J., Fast, D.E, and Strom, C.R. 2009. Effects of passive integrated transponder tags on smolt-to-adult recruit survival, growth, and behavior of hatchery spring Chinook salmon. N. Am. Fish. Man. 29:658-669.

Pacific Salmon Commission Joint Chinook Technical Committee. 1997. Incidental fishing mortality of Chinook salmon: mortality rates applicable to Pacific Salmon Commission fisheries. Report tcChinook (97)-1.

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Ricker, W.E. 1975. Computation and interpretation of biological statistics of fish populations. Bull. Fish. Res. Board. Can. 191.

Documents Reviewed:

O'Farrell, M.R., Mohr, M.S., and Grover, A.M. 2010. Sacramento River winter Chinook cohort reconstruction: analysis of ocean fishery impacts. NOAA Technical Memorandum (draft)

NMFS. 2010. ESA section 7 consultation initiation package: revised final (Jan 8 2010).

NMFS 2010. Biological opinion on the effects of ocean harvest of salmon on Sacramento River winter Chinook salmon Evolutionary Significant Unit (ESU).

Statement of Work

(T1016-04, v 20 July 2009)

External Independent Peer Review by the Center for Independent Experts

Biological Opinion on the effects of Ocean Harvest of Salmon on Central Valley Chinook Salmon Evolutionarily Significant Units (ESUs).

Scope of Work and CIE Process: The National Marine Fisheries Service's (NMFS) Office of Science and Technology coordinates and manages a contract providing external expertise through the Center for Independent Experts (CIE) to conduct independent peer reviews of NMFS scientific projects. The Statement of Work (SoW) described herein was established by the NMFS Project Contact and Contracting Officer's Technical Representative (COTR), and reviewed by CIE for compliance with their policy for providing independent expertise that can provide impartial and independent peer review without conflicts of interest. CIE reviewers are selected by the CIE Steering Committee and CIE Coordination Team to conduct the independent peer review of NMFS science in compliance the predetermined Terms of Reference (ToRs) of the peer review. Each CIE reviewer is contracted to deliver an independent peer review report to be approved by the CIE Steering Committee and the report is to be formatted with content requirements as specified in **Annex 1**. This SoW describes the work tasks and deliverables of the CIE reviewer for conducting an independent peer review of the following NMFS project. Further information on the CIE process can be obtained from www.ciereviews.com.

Project Description: This biological opinion will evaluate the impacts of Ocean Harvest of Salmon on ESA-listed Chinook salmon ESUs in California's Central Valley. The biological opinion will analyze issues such as level of take of listed ESUs vs. non-listed ESUs managed under the Salmon Fishery Management Plan of the Pacific Fishery Management Council, and the impact of this level of take on the survival and recovery of ESA-listed . The Terms of Reference (ToRs) of the peer review are attached in **Annex 2**.

Requirements for CIE Reviewers: Three CIE reviewers shall conduct an impartial and independent peer review in accordance with the SoW and ToRs herein. CIE reviewers shall have working knowledge and recent experience in the application of (1) salmon cohort reconstructions through the use of coded wire tag in assessment of salmon population abundance and distribution, and 2) salmon population ecology. Each CIE reviewer's duties shall not exceed a maximum of 10 days to complete all work tasks of the peer review described herein.

Location of Peer Review: Each CIE reviewer shall conduct an independent peer review as a desk review, therefore no travel is required.

Statement of Tasks: Each CIE reviewers shall complete the following tasks in accordance with the SoW and Schedule of Milestones and Deliverables herein.

Prior to the Peer Review: Upon completion of the CIE reviewer selection by the CIE Steering Committee, the CIE shall provide the CIE reviewer information (full name, title, affiliation, country, address, email) to the COTR, who forwards this information to the NMFS Project Contact no later the date specified in the Schedule of Milestones and Deliverables. The CIE is responsible for providing the SoW and ToRs to the CIE reviewers. The NMFS Project Contact is responsible for providing the CIE reviewers with the background documents, reports, and other pertinent information. Any changes to the SoW or ToRs must be made through the COTR prior to the commencement of the peer review.

Pre-review Background Documents: Two weeks before the peer review, the NMFS Project Contact will send (by electronic mail or make available at an FTP site) to the CIE reviewers the necessary background information and reports for the peer review. In the case where the documents need to be mailed, the NMFS Project Contact will consult with the CIE Lead Coordinator on where to send documents. CIE reviewers are responsible only for the pre-review documents that are delivered to the reviewer in accordance to the SoW scheduled deadlines specified herein. The CIE reviewers shall read all documents in preparation for the peer review.

This list of pre-review documents may be updated up to two weeks before the peer review. **Any delays in submission of pre-review documents or reports for the CIE peer review will result in delays with the CIE peer review process, including a SoW modification to the schedule of milestones and deliverables.** Furthermore, the CIE reviewers are responsible only for the pre-review documents that are delivered to the reviewer in accordance to the SoW scheduled deadlines specified herein.

Desk Review: Each CIE reviewer shall conduct the independent peer review in accordance with the SoW and ToRs, and shall not serve in any other role unless specified herein. **Modifications to the SoW and ToRs can not be made during the peer review, and any SoW or ToRs modifications prior to the peer review shall be approved by the COTR and CIE Lead Coordinator.** The CIE Lead Coordinator can contact the Project Contact to confirm any peer review arrangements.

Contract Deliverables - Independent CIE Peer Review Reports: Each CIE reviewer shall complete an independent peer review report in accordance with the SoW. Each CIE reviewer shall complete the independent peer review according to required format and content as described in Annex 1. Each CIE reviewer shall complete the independent peer review addressing each ToR as described in Annex 2.

Specific Tasks for CIE Reviewers: The following chronological list of tasks shall be completed by each CIE reviewer in a timely manner as specified in the **Schedule of Milestones and Deliverables**.

- 1) Conduct necessary pre-review preparations, including the review of background material and reports provided by the NMFS Project Contact in advance of the peer review.
- 2) Conduct an independent peer review in accordance with the ToRs (**Annex 2**).
- 3) No later than 9 February 2010, each CIE reviewer shall submit an independent peer review report addressed to the “Center for Independent Experts,” and sent to Mr. Manoj Shrivani, CIE Lead Coordinator, via email to shivlanim@bellsouth.net, and to Dr. David Die, CIE Regional Coordinator, via email ddie@rsmas.miami.edu. Each CIE report shall be written using the format and content requirements specified in Annex 1, and address each ToR in **Annex 2**.

Schedule of Milestones and Deliverables: CIE shall complete the tasks and deliverables described in this SoW in accordance with the following schedule.

<i>5 January 2010</i>	CIE sends reviewer contact information to the COTR, who then sends this to the NMFS Project Contact
<i>12 January 2010</i>	NMFS Project Contact sends the CIE Reviewers the report and background documents
<i>19 January – 2 February 2010</i>	Each reviewer conducts an independent peer review as a desk review
<i>9 February 2010</i>	CIE reviewers submit draft CIE independent peer review reports to the CIE Lead Coordinator and CIE Regional Coordinator
<i>23 February 2010</i>	CIE submits the CIE independent peer review reports to the COTR
<i>2 March 2010</i>	The COTR distributes the final CIE reports to the NMFS Project Contact and regional Center Director

Modifications to the Statement of Work: Requests to modify this SoW must be approved by the Contracting Officer at least 15 working days prior to making any permanent substitutions. The Contracting Officer will notify the COTR within 10 working days after receipt of all required information of the decision on substitutions. The COTR can approve changes to the milestone dates, list of pre-review documents, and ToRs within the SoW as long as the role and ability of the CIE reviewers to complete the deliverable in accordance with the SoW is not adversely impacted. The SoW and ToRs shall not be changed once the peer review has begun.

Acceptance of Deliverables: Upon review and acceptance of the CIE independent peer review reports by the CIE Lead Coordinator, Regional Coordinator, and Steering Committee, these reports shall be sent to the COTR for final approval as contract deliverables based on compliance with the SoW and ToRs. As specified in the Schedule of Milestones and Deliverables, the CIE shall send via e-mail the contract deliverables (CIE independent peer review reports) to the COTR (William Michaels, via William.Michaels@noaa.gov).

Applicable Performance Standards: The contract is successfully completed when the COTR provides final approval of the contract deliverables. The acceptance of the contract deliverables shall be based on three performance standards:

- (1) each CIE report shall be completed with the format and content in accordance with **Annex 1**,
- (2) each CIE report shall address each ToR as specified in **Annex 2**,
- (3) the CIE reports shall be delivered in a timely manner as specified in the schedule of milestones and deliverables.

Distribution of Approved Deliverables: Upon acceptance by the COTR, the CIE Lead Coordinator shall send via e-mail the final CIE reports in *.PDF format to the COTR. The COTR will distribute the CIE reports to the NMFS Project Contact and Center Director.

Key Personnel:

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Annex 1: Format and Contents of CIE Independent Peer Review Report

1. The CIE independent report shall be prefaced with an Executive Summary providing a concise summary of the findings and recommendations, and specify whether the science reviewed is the best scientific information available.
2. The main body of the reviewer report shall consist of a Background, Description of the Individual Reviewer's Role in the Review Activities, Summary of Findings for each ToR in which the weaknesses and strengths are described, and Conclusions and Recommendations in accordance with the ToRs.
3. The reviewer report shall include the following appendices:

Appendix 1: Bibliography of materials provided for review

Appendix 2: A copy of the CIE Statement of Work

Annex 2: Terms of Reference for the Peer Review

Biological Opinion on the effects of Ocean Harvest of Salmon on Central Valley Chinook Salmon Evolutionarily Significant Units (ESUs).

1. Evaluate the strengths and weakness of the cohort reconstruction analysis used to estimate the impact of fishing harvest on the ESUs considered in this Opinion.
2. Evaluate the interpretation of the coded wire tag recoveries and cohort reconstruction analysis, and any conclusions drawn about how these results are produced in light of how the fishery is managed.
3. How could the cohort reconstruction data or analysis be improved?
4. Are there additional quantitative or qualitative ways other than coded wire tag based methods to assess harvest impacts not considered in this Opinion?
5. Overall, does the biological opinion represent the best scientific information available?